Report on the 2006 Assessment of Shortbelly Rockfish in the California Current

NIWA Client Report: WLG2006-47
July 2006

NIWA Project: MIA06301

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Prepared for

The Center for Independent Experts

University of Miami

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Reviewed and Approved for release by:

Dr Andrew Laing

Executive Summary

A panel discussed the assessment of shortbelly rockfish in 2006. The Panel met 28-30 June 2006 at the Fisheries Ecology Division of NOAA/NMFS in Santa Cruz. The initial draft assessment was presented to the Panel, additional analyses were requested and carried out, and the Panel discussed the results.

This was a useful assessment which provided a rare and valuable opportunity to study the dynamics of an unfished population and question the assumption of stationarity which underlies most assessments of exploited stocks. It also demonstrated the value of a type of data not much used in fisheries stock assessments: that concerning food habits of predators such as sea birds and marine mammals.

Some recommendations are made concerning analyses that might be useful either in completing the current assessment or in approaching future assessments.

1. Background

This report reviews, at the request of the University of Miami (see Appendix 1), the 2006 assessment of shortbelly rockfish in the California Current. The author was provided beforehand with various documents (Appendix 2) and participated in the meeting which considered the assessment.

2. Review Activities

The review panel met 28-30 June 2006 at the Fisheries Ecology Division of NOAA/NMFS in Santa Cruz. Those attending the meeting included the assessment team, other participants from Santa Cruz, three external reviewers, and a representative of the Groundfish Advisory Panel (Appendix 3).

The initial draft assessment was presented to the Panel, additional analyses were requested and carried out, and the Panel discussed the results. Neither a Panel report nor a final assessment was produced, but the assessment team was given clear suggestions as to what sorts of assumptions should be considered in a final assessment.

3. Findings

3.1 Data

Some of the strengths and weaknesses of the available data sets are apparent even before any assessment modelling. For example, the sea lion length-frequency (LF) data show clear patterns of strong and weak year classes and are thus, on the grounds of this internal consistency, highly informative. The larval survey, which provides the only absolute biomass index, will also make a strong contribution. The murre food-habit index could be seen as of dubious value because of its restricted geographical range, but the fact that it correlates well with the index from the juvenile trawl surveys adds support to it. The triennial survey is not well-suited to a semi-pelagic species like shortbelly. This is of concern, because this survey provides quite a bit of data (indices and LFs) which may mislead the assessment model. These LFs do not show the clear internal consistency of those associated with sea lions (though it is harder to judge consistency with triennial data). The CalCOFI abundance index seems promising (shortbelly larvae occur in nearly half the stations in the standard grid) and is likely to be quite influential because it shows such a strong contrast (the mean value since 1959 is less than 25% of that for the preceding years). However, it assumes the larval production per mature female does not vary significantly from year to year, which may not be true. Least influential are the hydro-acoustic biomass estimates, which are relative (because the target strength of shortbelly is unknown), highly uncertain (with c.v.s of 0.5), only two in number, and close in time.

A major difficulty for the assessment was the limited and disparate geographic ranges of the data sets (see figure 1 in the draft assessment report). No data set consistently covered the nominal latitudinal range of the assessment, although the CalCOFI index did in some years. Some pairs of data sets were from non-overlapping areas. This should be taken as a prompt to check for conflict between data sets. The most obvious example in this assessment was the potential for conflict in recruitment fluctuations inferred from northern (murre index plus juvenile surveys) and southern (sea lion LFs) data sets. It is always difficult to know to what extent data from a limited area is representative of a broader area. A discussion on p. 9 of the draft assessment report indicates that it will sometimes be very non-representative.

One category of data that was used, in small amounts, but not much discussed during the review, was age frequencies (AFs). The data file in the draft assessment report contained three sources of AFs, each with just one year's data – hydro-acoustic, larval survey and (nominally) triennial – though I understand that at least some of these were sometimes switched off. I think it would be wise to be cautious with such data. With only single years we cannot use the criterion of internal consistency to test their validity. Given the limited range of other data, these AFs could be very influential in estimating the strength of individual year classes. The hydro-acoustic AF must be suspect since, presumably, it does not derive from random fishing. Doubt about the correct selectivity to apply to an AF might be a good reason to avoid using it.

Biomass indices from the food-habit data (murre and sea lions) present a difficulty because they derive from presence/absence observations. This makes them potentially sensitive to saturation (if shortbelly occur in, say, 60% of observations at a given level of abundance, that occurrence can not double if the abundance does). Thus, some sort of transformation is needed to make these indices proportional to abundance. A related problem occurs with the two 0+ indices (murre and juvenile survey), which concern fish of such an early age that they could well still be subject to density-dependent natural mortality. If they are, a transformation is again suggested to induce proportionality. An obvious choice offered by Stock Synthesis II (SS2) is the power transformation (this has one parameter, called the power parameter for catchability). I will say more about estimating these transformations below.

After the review meeting I noticed what appears to be an error in the part of the data file associated with LFs from CalCom and the sea lion data. This error may well have been corrected, without comment, during the review meeting, but I mention it here in case it was overlooked. For both of these data sets gender was set to 3, implying that separate male and female proportions were available, although the male and female proportions were identical. My reading of the SS2 User Manual suggests that gender should have been set to 0 to signal that sex was not observed.

3.1.1 GLMs

I was interested in the extensive use of generalised linear models (GLMs) in the construction of biomass indices for this assessment, and a little uneasy about some of this. For the food habit data, I have no problem. GLMs are an obvious technique for removing some noise (e.g., due to seasonal changes) from such data. However, there does seem to be some unresolved problem with the sea lion index, for which the year-to-year variation is much smaller than is plausible, given the rather high error estimates and the high recruitment variability indicated by the associated LFs.

It is with the spatially extensive surveys (CalCOFI, triennial, and juvenile) that I am more concerned, and my concern is that the GLM assumptions are very strong, and perhaps unwarranted. I understand that the juvenile surveys used to be analysed using the more conventional stratified-random assumptions but, after careful consideration, it was decided to switch to the GLM approach some years ago. I have not seen the analyses underpinning this decision, and so am not able to say whether I would find them convincing. I would like just to sound some notes of caution about the use of GLMs for these surveys.

First, with regard to the triennial survey, I would urge consideration of the impossibility of creating silk purses from sow's ears. Bottom trawl surveys are notoriously poor at indexing semi-pelagic species like rockbelly, and the use of GLMs cannot make them any better. GLMs will generally reduce c.v.s, possibly substantially, but we may be deluding ourselves if we believe this indicates a true reduction in uncertainty. On the matter of uncertainty, I think we should have much more certainty in the CalCOFI index for years in which the survey covered the extended grid, than we have for years confined to the original restricted area. The fact that this difference in confidence is not seen in the GLM c.v.s seems, to me, to indicate a weakness in their derivation.

Second, like most models of biological processes, those constructed for these surveys using GLMs will be gross simplifications of reality. We should not assume that because a factor or interaction is found to be *statistically* insignificant, that it is *functionally* insignificant. We may have very little power to detect some of these, and my concern is that this might bias our estimated year effects. Year-area interactions are certain to occur (the penultimate paragraph on p. 9 of the draft assessment report describes one such) but may or may not be detected. Of course, any interactions with year can be difficult to deal with (because there's no longer a single year effect). Such interactions are automatically dealt with under the simpler assumptions for stratified random surveys.

3.2 Modelling

I was, in general, happy with the modelling approach adopted in this assessment. There was an initial attempt at a coast-wide model and then, when there appeared to be conflict between the north and south recruitment signals, separate models were constructed for these two regions. In this section I present some comments on two important aspects of the modelling.

3.2.1 Estimating the stock-recruitment relationship

In this assessment there were two parameters defining the stock-recruitment relationship: B_0 (or, equivalently, R_0) and h (steepness). Most of what I have to say on these concerns the former parameter. However, I would like to say that in my experience there are very few stock assessments (not including the current one) in which there is clearly sufficient information to estimate steepness. I would recommend that the final assessment for shortbelly include runs in which h is fixed to a suitable default value.

An unusual aspect of this assessment was the comparative lack of information to scale the biomass (i.e., to estimate B_0). In most assessments, trends in biomass are driven by the historical catches, so the assessment infers B_0 by addressing the question "How large must B_0 be to have allowed the historical catches to have caused the trends in biomass (or absolute biomass estimates) that have been observed?". This question seems inappropriate in the current assessment, since there is no fishery for shortbelly rockfish. All biomass fluctuations are effectively assumed to have been driven by variation in recruitment alone, which means that biomass trend data (e.g., from the CalCOFI and triennial surveys) contains no information about B_0, which is determined solely by the single absolute biomass estimate (from the 1991 larval production survey).

It is important to be clear about the meaning of B_0 in stock assessment models. This is widely misunderstood as being the biomass that existed before fishing began (sometimes referred to as the virgin biomass), which is misleading in two ways. First, fish stocks fluctuate, even in the absence of fishing, so that the only sensible definition of B_0 is as the theoretical level about which the biomass would fluctuate in the absence of fishing. Second, it is common in assessment models (including SS2) to force recruitment deviations to average zero (in log space). This means that R_0 is effectively defined to be the average recruitment over all years in which recruitment is estimated (after correction for the stock-recruit relationship). Now B_0 is calculated as the theoretical biomass that would occur if recruitment was constant at R_0 and there was no fishing. Thus, rather than thinking of B_0 as being associated with the period before fishing, we should think of it as being associated with the years over which recruitment is estimated in the assessment. It is, in some sense, the 'average' biomass that would have occurred over that period had there been no fishing.

There are two important consequences of this view of B_0. First, it shows how artificial it is to set the initial biomass equal to B_0 in stock assessments. This may be defensible, on the grounds of parsimony, in the assessment of a stock for which the historical catches are believed to have had a much greater effect on the stock biomass than has recruitment variation. However, it makes no sense in the current assessment, where all variation is assumed to be due to changes in recruitment and the initial biomass could easily have been well above, or well below, B_0. I believe that the decision, made during the current assessment meeting, to allow $B_{\text{init}} \neq B_0$, will have a strong impact on the estimate of current depletion (as measured by the ratio B_{current}/B_0). A second consequence is to highlight a weakness of SS2. It is common to estimate recruitment for a wide range of years, but to have reliable recruitment information only for a narrower range. It will often make sense to use only this narrower range of years in defining B_0 (i.e., to force recruitment deviates to average zero only over this narrower range). A useful extension to SS2 would be to allow users to be able to specify the range of years used to define R_0 (and thus B_0), and to allow this to be different from (narrower than) the range of years for which recruitments are estimated. Such a distinction is available in the assessment program CASAL (Bull et al. 2005).

3.2.2 'Tuning' the model

The term 'tuning' was used to describe two different activities during the review meeting. The first was the estimation of transformations to deal with either saturation (for binomial indices) and/or density-dependent mortality (for 0+ indices) (see Section 3.1 above). The second was the process of iterative reweighting to change the relative emphasis placed on different data sets. While I agree in general with the application of both of these techniques, I would like to counsel caution in their use. The main point I'd like to make is that both require some sort of fixed point, or fulcrum, against which to gain leverage in estimation. In the absence of a suitable fulcrum these techniques are better not used.

In the coast-wide assessment, it seemed to me that the fulcrum needed to estimate density-dependent mortality was the sea lion LF data set. This suggested greater variation in recruitment than was indicated by either of the two 0+ indices. Thus, the model had a clear signal to use in estimating catchability power parameters for the two 0+ indices. However, I see no future in trying to estimate these parameters in the north-only model, where there seemed to be no such fulcrum. Nor do I see any point in trying to estimate both saturation and density-dependent mortality, which are confounded in this assessment. The best I think that can be done is to estimate a single parameter which allows for the joint effect of these two processes.

When in doubt about the existence of a plausible fulcrum I suggest profiling on the power parameter(s). In the case of the coast-wide model, I would expect this to identify the sea lion LF data as the fulcrum by showing how, as the power parameters depart from their null values, the fit to this data set degrades, and the fit to the 0+ indices improves.

With regard to iterative reweighting, I think the required fulcrum is usually a subset of the data sets whose error c.v.s (or effective sample sizes) are pre-judged to be already reasonable; the smaller the collection of data sets that is to be reweighted, the better. Another point to be made is that reweighting is better suited to large data sets (usually LFs or AFs). For a biomass index with only 10 or 20 observations it is difficult to say whether a mismatch between the estimated and expected values of rmse (root-mean-square error) is an indication of incorrect c.v.s (i.e., a need to reweight) or just a poor estimate of rmse from a small sample.

Ideally, stock-assessment decisions should be objective. In practice, this is often not possible, and I believe it is quite reasonable to intervene in an iterative reweighting to ensure that its effect is not counter to the expert judgement of the scientists involved. In other words, it is proper to prevent the up-weighting of data sets that are believed to be suspect and/or the down-weighting of those that are thought to be reliable. I suspect that such an intervention was needed at times in the shortbelly assessment. In recent hoki assessments in New Zealand, the model has had difficulty in fitting the strong downward trend in a particular trawl survey index (the lack of fit being indicated as much by a trend in residuals as by an rmse that was too high). Rather than increasing the c.v.s for this data set (as would be suggested by iterative reweighting), the Hoki Fisheries Assessment Working Group decided it would be better to do the opposite, to ensure that the model better reflected what was deemed to be an important signal in the data (Francis 2006b). That seems to me a quite proper intervention.

3.2.3 Contribution of individual data sets

One of the important tasks of those involved in stock assessments is to gain an understanding of the contribution of each data set. We need to understand which data sets are influential, and amongst influential data sets, which model outputs they are influencing (and in which direction). This information, in conjunction with some idea of the reliability of each data set, is important in interpreting the assessment. To this end, there are two techniques that I would recommend.

The first is a sensitivity analysis which successively leaves out one data set at a time. This quickly identifies data sets with little influence and is most useful with minor data sets (e.g., the AFs in the current assessment). The second is profiling on key parameters. This helps to understand how well such parameters are determined, and what compromise is involved in their estimation (i.e., which data sets 'prefer' a lower or higher value of the parameter, and which data sets are 'indifferent' to it).

One particular reason for mentioning these techniques is the very high recruitment that was estimated for 2003 (I think) in some of the later model runs presented to the review. My suspicion is that this estimate was driven by the peak of small fish in the LF from the 2004 triennial survey, and is probably unreliable.

4. Conclusions

4.1 The assessment

This was an interesting assessment which provided a rare and valuable opportunity to study the dynamics of an unfished population. To some extent our approach to the management of fisheries is based on the assumption that we understand the behaviour of populations that are not fished. Assessments like this allow us to examine that assumption. Although the data presented some problems (concerning representativeness and areal coverage), these were no greater than is common in many fisheries assessments that are deemed adequate for use in managing stocks.

The food habit data from sea lions and murres made important contributions to the assessment. Although there was some doubt about the abundance index derived from the sea lion data (see above), the associated length frequencies were clearly informative, and the murre 0+ index seemed also to be useful. It would be worthwhile to consider whether such food habit data could be useful in other assessments. In the present assessment these data are important in suggesting north-south differences in recruitment.

Some aspects of this assessment question the assumption of stationarity that underlies most stock assessments. As an example of the use of this assumption note that estimates of depletion (which determine whether a stock is deemed to be overfished) are based on the assumption that the relationship between the spawning stock and recruitment (and thus the definition of B_0) does not vary with time. Both 0+ indices used in this assessment suggest that recruitment after about 1990 was lower, by around one order of magnitude, than that before that date. This pattern is supported by a decline in the CalCOFI index through the 1990s. Another apparent non-stationarity is evident in the early part of the CalCOFI index (the mean value since 1959 is less than 25% of that for the preceding years). Finally, I note that there is some evidence of a substantial increase in sea lion abundance over the period covered by this assessment (I understand that current pup counts are an order of magnitude higher than those in 1975). This raises doubt over the assumption that natural mortality is time invariant. Clearly, any conclusions that might be drawn from the results of this assessment will depend strongly on how we interpret these indications of non-stationarity.

4.2 Future work

I conclude by mentioning several analyses that I think would be worth pursuing, either in completing the current assessment, or in future assessments. Some other suggestions are included in the preceding text.

It may be worth bootstrapping the triennial LFs to get an idea of how well these are determined. When this was done recently with LFs based on observer data in the New Zealand orange roughy fishery, strong correlations were found within the LFs (i.e., p_{iy} was strongly correlated with p_{jy}, where p_{iy} and p_{jy} are the estimated proportions in the ith and jth length bins in year y) (Francis 2006a). The effect of these correlations was to make the mean length for each LF much more uncertain than would be implied from the bootstrap-estimated c.v.s for the individual LF proportions, p_{iy}. The effect of this analysis of the triennial LFs could be to suggest their down-weighting in the assessment.

The shortbelly growth curve should be re-estimated after appropriate fractional ages have been assigned to all observations. The data used for the assessment were treated as if all age-length observations were made in the middle of the year (i.e., a fractional age of 0.5 y was assumed for all observations), which I understand not to have been the case. This reanalysis may lead to a better fit to the left-hand ends of LFs in the stock assessment.

It would be useful to obtain some measure of uncertainty for the areal-expansion factor used to scale the larval-production biomass estimate up to the total area for the coast-wide assessment. This will have no effect on the point estimates from the assessment. However, it would affect uncertainty estimates obtained either from the inverse Hessian or from profiling key parameters.

If there is to be extensive use in West Coast assessments of LFs that are inferred (e.g., from otoliths in sea lion scat) rather than observed, it might be worth extending SS2 to include an associated error matrix (analogous to the ageing-error matrices). On this topic, I wonder how the conversion was made from otolith length to fish length. I am assuming that a one-to-one mapping was made between each otolith length measurement and the most probable fish-length bin. If so, that might explain why the observed sea lion LFs appeared to be more peaked than those estimated in the assessment model. Perhaps a better way would be to assign an LF probability distribution for each otolith length measurement.

Future assessments of shortbelly rockfish should consider using information about long-term changes in the abundance of key predators (particularly sea lions and murres) to drive changes in the mortality caused by these predators.

APPENDIX 1: Statement of Work

This appendix contains the Statement of Work that formed part of the consulting agreement between the University of Miami and the author.

Rationale

The Center for Independent Experts (CIE) has participated extensively in the Stock Assessment Review (STAR) Panels developed for West Coast groundfish stock assessments in 2005. The Fisheries Ecology Division (FED), at the Southwest Fisheries Science Center conducted an additional assessment that was not requested by the Pacific Fisheries Management Council (PFMC), and was not a part of the traditional stock assessment review process. This assessment was done on the shortbelly rockfish, *Sebastes jordani*, an unfished but ecologically important species with the distribution centered off of southern and central California. There is evidence to suggest that the population has undergone significant fluctuations in abundance over the last several decades, presumably in response to variations in ocean conditions. As this unfished stock might be considered the equivalent of a "control" rockfish population, the results of this assessment may be informative with regard to the understanding the potential causes and consequences of natural population variability on exploited rockfish populations throughout the California Current.

General

External, independent review of West Coast groundfish stock assessments has been an essential part of the fisheries management process. However this review is not being conducted on an assessment that is intended to directly provide management advice, as there are currently no important management decisions to be made for this unexploited species. Because the Council review process could not fit this assessment into the standard STAR review schedule, we have sought the opportunity to review this assessment separately. As such, this assessment is not intended to provide the basis for management of groundfish on the West Coast in the short term (tactical) sense; rather it is intended to investigate the role that ecosystem interactions may play in the management of west coast fisheries.

This review (including Terms of Reference, etc.) will be similar to reviews conducted under the West Coast STAR process (as described in the PFMC Terms of Reference, to be provided), however with a smaller number of reviewers. Currently, we anticipate two to three independent reviewers on this panel, one of whom will be the CIE reviewer. Groundfish Management Team (GMT) and Groundfish Advisory

Subpanel (GAP) advisors from the PFMC would not be formally included in this review panel, however representatives from both of these advisory bodies will be invited to participate.

The CIE expert should have experience in population dynamics and stock assessment of groundfish, and past experience reviewing west coast groundfish assessments would be beneficial. The expert should have specific experience in the integrated analysis type of modeling approach, using ADMB, age-and size-structured models, use of MCMC to develop confidence intervals, and use of Generalized Linear Models to process survey and logbook data for use in assessment models. Although the modeling framework for this model is the same as most of the west coast groundfish models reviewed in 2005 (e.g., a maximum likelihood modeling framework using Stock Synthesis 2, SS2), the focus of the assessment and the types of data used in the assessment diverge modestly. For example, the model uses information from both larval and juvenile abundance surveys (both of which have been used in other west coast groundfish assessments) as well as seabird and sea lion food habits studies (which have not been used in past assessments). Similarly, the authors have devoted more time and effort to understanding and quantifying past population trends, rather than identifying potential future yields, again based on the fact that the assessment was not formally requested by the PFMC.

Documents to be provided to the CIE reviewer prior to the review include the following:

- Current draft stock assessment reports;
- Most recent previous stock assessments and reviews;
- Groundfish Stock Assessment and Review Process Terms of Reference;
- An electronic copy of the data, the parameters, and the model used for the assessments (if requested by reviewer);
- Additional supporting documents, primarily in the form of subset of published research papers directly relevant to this effort.

The reviewer's duties shall not exceed a maximum of 14 days: several days prior to the meeting for document review; a two and one-half day meeting to review the documentation, model, and model results; and several days following the meeting to complete the written report. The meeting will be held at the main conference room of the Southwest Fisheries Science Center's Santa Cruz Laboratory, between June 28 and June 30, from 8:30 a.m. to 5:00 pm for the first two days, and from 8:30 a.m. to 12:30 p.m. on the last day. The format of the meeting will consist of an initial presentation of data sources, modeling assumptions and model results, followed by discussions of the different approaches and the opportunity to alter the model and/or conduct new analyses as appropriate. The CIE reviewer's report is to be based on the reviewer's findings. The reviewer's tasks consist of the following:

1) Become familiar with the draft stock assessments and background materials for the model.

2) Actively participate in the review.

3) Comment on the primary sources of uncertainty in the assessment.

4) Comment on the strengths and weaknesses of current approaches.

5) Recommend alternative model configurations or formulations as appropriate.

6) No later than July 14, 2006, submit a written report[1] consisting of the findings, analysis, and conclusions to Dr. David Die, via email to ddie@rsmas.miami.edu, and to Mr. Manoj Shivlani, via email to mshivlani@rsmas.miami.edu. See Annex 1 for additional details on the report contents and organization.

Annex 1: Contents of the Reviewer's Report

1. The report shall be prefaced with an executive summary of findings and/or recommendations.

2. The main body of the report shall consist of a background, description of review activities, summary of findings (addressing the issues raised in this statement of work), and conclusions/recommendations.

3. The report shall also include as separate appendices the bibliography of all materials provided by the Center for Independent Experts and a copy of the statement of work.

[1] The written report will undergo an internal CIE review before it is considered final. After completion, the CIE will create a PDF version of the written report that will be submitted to NMFS and the reviewer.

APPENDIX 2: Materials Provided

Before the review the Panel was provided with electronic copies of the following documents.

The assessment

Field, J.C.; Dick, E.J.; MacCall, A. (2006).Technical description of a population model for the shortbelly rockfish, *Sebastes jordani*, in the California Current (draft dated June 20 2006).

STAR panel process

Anonymous (undated) Groundfish stock assessment and review process for 2005-2006. [Includes terms of reference for STAR panels]

SS2 documentation

Methot, R.D. (2005). Technical Description of the Stock Synthesis II Assessment Program Version 1.17 – March 2005.

Methot, R.D. (2005). User Manual for the Assessment Program Stock Synthesis 2 (SS2) Model Version 1.19 April 27, 2005.

Other papers

Lowry, M.S. & Carretta, J.V. (1999). Market squid (/Loligo opalescens/) in the diet of California Sea Lions (/Zalophus californianus/) in southern California (1981-1995). Calif. Coop. Oceanic Fish. Invest. Rep, 40, 196-207.

Miller, A.K. and W. Sydeman. 2004. Rockfish response to low-frequency ocean climate change as revealed by the diet of a marine bird over multiple time scales. Mar. Ecol. Progr. Ser. 281: 207-216.

Mills, K.L., T. Laidig, S. Ralston and W.J. Sydeman. In prep. Diets of top predators indicate pelagic juvenile rockfish (Sebastes spp.) abundance in the California Current System

Moser, H.G., R.L. Charter, W. Watson, D.A. Ambrose, J.L. Butler, S.R. Charter, and E.M. Sandknop. 2000. Abundance and distribution of rockfish (/Sebastes/) larvae in the southern California Bight in relation to environmental conditions and fishery exploitation. Calif. Coop. Oceanic Fish. Invest. Rep. 41: 132-147.

Pearson, D.E., J.E. Hightower, and J.T.H. Chan. 1991. Age, growth, and potential yield for shortbelly rockfish Sebastes jordani. Fish. Bull. 89: 3: 403-409.

Ralston, S., J.R.Bence, M.B. Eldridge, and W.H. Lenarz. 2003. An approach to estimating rockfish biomass based on larval production, with application to Sebastes jordani. Fish. Bull. 101:129-146.

APPENDIX 3: List of Participants

Participants in the review meeting included the following

Assessment team

John Field, E.J. Dick, Alec MacCall

Other participants from Santa Cruz

Xi He, Meisha Key, Stephen Ralston

External reviewers

Chris Francis, Bill Lenarz, George Watters

Groundfish Advisory Panel representative

Tom Ghio